AD-A093 208 YALE UNIV NEW HAVEN CT DEPT OF PSYCHOLOGY F/G 5/10
NOTHING FAILS LIKE SUCCESS: THE SEARCH FOR AN INTELLIGENT PARAD--ETC(U)
OCT 80 R J STERNBERG N00014-78-C-0025
UNCLASSIFIED RR-8-80 NL

1 of 1

END
DATE
FILMED
1-81
DTIC

AD A093208

LEVEL IV

12 NW

Nothing Fails Like Success:

The Search for an Intelligent Paradigm for Studying Intelligence

Department of Psychology
Yale University
New Haven, Connecticut 06520

Technical Report No. 29
October, 1980

This research was sponsored by the Personnel and Training Research Programs, Psychological Sciences Division, Office of Naval Research, under Contract No. N0001478C0025, Contract Authority Identification Number NR 150-412.

DISCLAIMER NOTICE

THIS DOCUMENT IS BEST QUALITY PRACTICABLE. THE COPY FURNISHED TO DTIC CONTAINED A SIGNIFICANT NUMBER OF PAGES WHICH DO NOT REPRODUCE LEGIBLY.

(14) KK-8-80, TR-27

UNCLASSIFIED

SECURITY CLASSIFICATION OF THIS PAGE (When Data Entered)

REPORT DOCUMENTATION PAGE		READ INSTRUCTIONS BEFORE COMPLETING FORM
1. REPORT NUMBER Technical Report No. 29	2. GOVT ACCESSION NO. AD-A093 208	3. RECIPIENT'S CATALOG NUMBER (9) Research rept.
4. TITLE (and Subtitle) (6) "Nothing Fails Like Success: The Search for an Intelligent Paradigm for Studying Intelligence"		5. TYPE OF REPORT & PERIOD COVERED Periodic Technical Report 1 Jul 80 — 30 Sep 80 6. PERFORMING ORG. REPORT NUMBER Research Report No. 8-80
7. AUTHOR(s) (10) Robert J. Sternberg		8. CONTRACT OR GRANT NUMBER(s) N0001478C0025
9. PERFORMING ORGANIZATION NAME AND ADDRESS Department of Psychology Yale University New Haven, Connecticut 06520		10. PROGRAM ELEMENT, PROJECT, TASK AREA & WORK UNIT NUMBERS 61153N; RR 042-04; RR 042-01; NR 150-412
11. CONTROLLING OFFICE NAME AND ADDRESS Personnel and Training Research Programs Office of Naval Research (Code 458) Arlington, Virginia 22217		12. REPORT DATE (11) 1 Oct 80 (12) 57 13. NUMBER OF PAGES 38
14. MONITORING AGENCY NAME & ADDRESS (if different from Controlling Office) (15) N00014-78-C-0025 (16) KK04204		15. SECURITY CLASS. (of this report) Unclassified 15a. DECLASSIFICATION DOWNGRADING SCHEDULE

16. DISTRIBUTION STATEMENT (of this Report)

Approved for public release; distribution unlimited

17 RR04201

17. DISTRIBUTION STATEMENT (of the abstract entered in Block 20, if different from Report)

18. SUPPLEMENTARY NOTES

Journal of Educational Psychology, in press

19. KEY WORDS (Continue on reverse side if necessary and identify by block number)

Intelligence, cognitive-components analysis, cognitive-correlates analysis, factor analysis

20. ABSTRACT (Continue on reverse side if necessary and identify by block number)

The possibility is considered that research on intelligence is entering or is about to enter a time of crisis comparable to that experienced during the decline of the psychometric paradigm as the primary means for studying intelligence. First, it is suggested that the decline of the psychometric paradigm as the primary means for studying intelligence was due in part to the failure of users of the paradigm to meet in a highly successful [illegible] challenges that confronted their research. Next, it is shown how [illegible]

DD FORM 1 JAN 73 1473 EDITION OF 1 NOV 65 IS OBSOLETE UNCLASSIFIED

S/N 0102-LF-014-6601 402628

SECURITY CLASSIFICATION OF THIS PAGE (When Data Entered)

surface, users of the information-processing paradigms currently in favor seem successfully to have met these challenges. Then, it is shown that, at a deeper level, the level of success is not as great as it is at the surface level. Finally, conclusions are drawn in response to the challenges that still seem to be facing psychologists studying intelligence.

Nothing Fails Like Success:

The Search for an Intelligent Paradigm for Studying Intelligence

Robert J. Sternberg

Yale University

Running head: Nothing Fails Like Success

Send proofs to Robert J. Sternberg
Department of Psychology
Yale University
Box 11A Yale Station
New Haven, Connecticut 06520

Abstract

The possibility is considered that research on intelligence is entering or is about to enter a time of crisis comparable to that experienced during the decline of the psychometric paradigm as the primary means for studying intelligence. First, it is suggested that the decline of the psychometric paradigm as the primary means for studying intelligence was due in part to the failure of users of the paradigm to meet in a highly successful way four challenges that confronted their research. Next, it is shown how, on the surface, users of the information-processing paradigms currently in favor seem successfully to have met these challenges. Then, it is shown that, at a deeper level, the level of success is not as great as it is at a surface level. Finally, conclusions are drawn in response to the challenges that still seem to be facing psychologists studying intelligence.

Nothing Fails Like Success:

The Search for an Intelligent Paradigm for Studying Intelligence

I believe the time has come at least to consider the possibility that research on intelligence is entering or is about to enter a time of crisis and soul-searching comparable to that experienced during the nineteen fifties and sixties, when researchers experienced certain dissatisfactions with the psychometric paradigm as the sole or primary means for studying intelligence, but were not quite sure of what should replace or supplement it. My contention is that at one level--the level that meets the eye upon a superficial examination of the present condition of intelligence research--current paradigms for studying intelligence have successfully faced the problems that factor analysis seemed to face in a less than wholly successful way, but that at another level--the level that meets the eye upon a deeper examination of the present condition of intelligence research--current paradigms are not facing these problems altogether successfully.

My exposition is divided into three parts. In the first, I state what I believe to have been four of the major challenges that the psychometric paradigm for studying intelligence, in general, and the factor-analytic approach, in particular, failed fully to meet. In the second part, I first show why, at a superficial level, at least, current approaches based upon the information-processing analysis of intelligent behavior are seeming to meet these challenges; I then show why, at a deeper level, I believe all of these challenges have yet to be confronted head on. In the third and final part, I discuss what might be done to meet these challenges. Because of the seemingly negative tone of many earlier parts of the article, I wish to emphasize here that these conclusions will be optimistic--that although all known methods have their limitations, intelligent use of a variety of methods can

result in major advances in our knowledge concerning the nature of intelligence.

The Four Challenges and Factor Analysis

Factor analysis has been and continues to be a highly useful tool for studying intellectual functioning: Nothing I will say is able or intended to refute this assertion. But factor analysis, like any other method of data analysis, is unable to go it alone. I believe that in the case of factor analysis, there are four reasons why supplementary methods of analysis are particularly important in the study of intelligence.

First, as Humphreys pointed out almost two decades ago, factor analysis is a "useful tool in hypothesis formation rather than hypothesis testing" (Humphreys, 1962, p. 475). Factor analysis is useful in hypothesis formation because one can go into it with few or even no ideas about the structure underlying a set of variables, and come out of it with at least some idea of what this latent structure looks like. I believe that nonconfirmatory factor analysis is not useful in hypothesis testing, however, because the inferential machinery supporting it is so weak.[1] There have been, of course, prominent investigators who have taken and still would take issue with this point of view. Burt (despite his apparent proclivity toward "assisting" his data, a competent factor analyst nevertheless) argued that factor analysis should be regarded "not as a source of hypotheses, but merely as a method of comparing, confirming or refuting alternative hypotheses initially suggested by nonstatistical arguments or evidence" (Burt, 1970, p. 17). More recently, Carroll (in press) has argued that "the machinery of factor analysis need not be dependent on any hypotheses adopted in advance of the analysis; actually it affords a way of testing those hypotheses," a way that Carroll believes to be "appropriate and sufficiently objective." But I cannot think of a single plausible psychological hypothesis whose validity has been conclusively tested and established (or disestablished) through the use of

(nonconfirmatory) factorial methods, despite the fact that these methods have been around for three-quarters of a century. Consider, for example, the very basic question of whether there is a general factor in intelligence. Almost eighty years after the first presentation of Spearman's (1904) two-factor theory, has anyone answered through factorial means the question of whether or not a general factor exists? The contents of a set of 16 commentaries on an article I recently wrote (Sternberg, in press-c) make it clear that no one has: Investigators disagree as much now as they did at the turn of the century as to whether to interpret factorial evidence as supporting or refuting the existence of a general factor.

Because of the weakness of its inferential machinery, factor analysis has, in a sense, failed because it has been too successful in supporting, or at least in failing to disconfirm, *too many* alternative models of intelligence. Horn (1967; Horn & Knapp, 1973) has suggested that Guilford's (1967) theory, in particular, is exceedingly difficult to disconfirm because of the way in which procrustean rotation is used. I have gone further in suggesting that none of the factorial theories are disconfirmable, because in major respects, all of them are correct (Sternberg, 1980a)! They highlight different aspects of intelligence, all of which can be mapped into information-processing terms. On the positive side, therefore, the theories have provided a richly variegated account of human intelligence; on the negative side, however, I do not see how intelligent use of factorial methods can fail to yield a legitimate theory. But science in general, and psychology in particular, progress at least in part by tentatively accepting certain accounts of phenomena, and by tentatively rejecting others; factor analysis has left us little, if anything, to reject. What plausible

theories of intelligence have been disconfirmed by factor analysis? My contention is that not only are the major alternative factorial theories mathematically tenable; they are psychologically tenable as well. The method might therefore be viewed as having failed, in practice, because it has been too successful!

Second, factor analysis has never seemed to be a technique of choice if one's goal is to identify the processes that constitute intelligent behavior. Factor analysis has dealt primarily with products rather than with processes. Even in recent work using confirmatory methods (e.g., Frederiksen, 1980), identification of processes has been through standard information-processing techniques, such as the subtraction method and the additive-factor method. Confirmatory analysis, e.g., analysis of covariance structures (Jöreskog, 1970), has been used to isolate common sources of individual differences in execution of these processes (which could include as well as common processes, common representations of information, common input modalities, common psychological units of analysis, etc.). Perhaps potentially, factor analysis might have told us or still might tell us more about information processing than it has. The past evidence, however, suggests that it is not a useful technique for separating process from other sources of individual differences, such as content. In Thurstone's (1938) theory, for example, the distinction between process and content is not clear. The one theory in which the distinction is very clear is Guilford's, but I suspect this reflects more Guilford's creative conceptualization of intelligence than the results of factor analyses performed on Guilford's or others' data (see the papers by Horn cited earlier). The fact of the matter is that at least until now, factor analysis has not been at its best in elucidating the processes constituting intelligent performance.

Third, by the end of the sixties, it seemed as though nonconfirmatory analysis had told us pretty much what it was going to tell us about the nature of in-

telligence. This view is not a negative assessment of the cumulative contribution of factor analysis; rather, it represents a belief that after three-quarters of a century, nonconfirmatory versions of the technique, at least as they have been used in the past for the analysis of IQ-test items, have pretty much been milked dry. Carroll (1980) has suggested that

> factor analysis is not at all as 'indeterminate' as it is often depicted to be, and as it was in fact depicted by Sternberg in his book (1977). With well-designed studies, the principles of simple structure can pretty well dictate the final solution. Parsimony is the essential principle underlying the idea of simple structure; it says that one wants to account for a given variable with the minimal number of factors--often with only one factor.

I do not agree with Carroll that the issue of a preferred rotation is dictated by anything, other than the mathematical constraints of rotation, which allow an infinity of valid rotations rather than a single one. I also consider parsimony to be only one of a number of criteria one should use in assessing the value of a factorial theory, or any other kind of theory, for that matter.(See Sternberg, 1977, Chapter 5, for a discussion of some other criteria--completeness, specificity, generality, plausibility--that matter as well.) And certainly, the issue of a preferred rotation (like, it seems, many other issues pertaining to factor analysis) remains a matter of debate. Contemporary factor theorists other than Carroll (e.g., Cattell, 1971; Guilford, 1967; Horn, 1968; Snow, 1978; Vernon, 1971; to name a few) continue to use solutions other than simple-structure ones in their theorizing about intelligence. But if the issue of a "correct" rotation had been decided, then I would be even more convinced that tradi-

tional factorial techniques have not recently been telling us a great deal new about the nature of intelligence: I don't believe we've learned a great deal more about the simple-structure nature of intelligence than we knew from Thurstone's early investigations of it (e.g., Thurstone, 1938)! I am not stating a belief that simple-structure solutions do not provide us with useful information about intelligence. To the contrary, I believe that these solutions, and others as well, provide us with a great deal of useful information (see Sternberg, 1980a , in press-$_c$). But I do not see that with the coming of the nineteen eighties, they are likely to provide us with much <u>new</u> information, unless they are applied to new materials or otherwise applied in new ways.

Fourth and finally, I believe that by the end of the nineteen sixties, factor-analytically derived theories were being perceived as less informative than might have been hoped with regard to their implications for instruction, in particular, instruction in intelligent information processing. Presumably, these theories were less helpful in this regard than one might have hoped because they did not make clear just what it is that should be trained. Obviously, one could train subjects in their performance on the items that compose the factors. But although the theories may delimit the class of items to be trained, they do not indicate how to train performance on these items. Instead of decomposing the items into smaller, more concrete and potentially trainable units, they relate the items to factors that are larger, more abstract, and probably less trainable than the items. It is easier to see, for example, how one might train analogy performance than it is to see how one might train performance on a factor of "reasoning" or of "general intelligence." One possible exception to this generalization (and there may well be others) is Guilford's (1967)

theory, where the factors seem to specify fairly elementary processes, contents, and products. Here, oddly enough, the problem might not be in the paucity of implications for training, but in their plethora: There are 120 abilities postulated in the model.

The Four Challenges and Information-processing Analysis

An Optimistic View

At first inspection, contemporary information-processing approaches to intelligence seem to be doing admirably in meeting squarely the challenges factor analysis met less than adequately. Indeed, contemporary approaches to the study of intelligence were formulated at least in part to mitigate these and other apparent "inadequacies" of factor analysis used in the absence of converging methods of analysis.

First, the methods of data analysis used in information-processing investigations of intelligence are highly useful in hypothesis testing (although probably less useful in hypothesis formation). Although the major statistical methods used in such investigations--analysis of variance and regression--are based upon the same general linear model upon which factor analysis is based, inferential statistics for the former two methods are much more highly developed than they are for factor analysis. One could, of course, argue over the potentials for hypothesis testing of factor analysis as opposed to analysis of variance or multiple regression. But to a large extent, the "proof of the pudding is in the tasting," and even a perfunctory review of the two literatures will reveal a much greater emphasis upon, and more successful use of, hypothesis testing in the information-processing literature than in the factor-analytic one. In one kind of information-processing analysis, computer simulation, the use of inferential statistics is often at a minimum. But even here, strict hypothesis testing is possible, albeit hypothesis testing of a different kind: The simulator

can test whether his or her program is a sufficient account of behavior simply by seeing whether the program (a) runs and (b) produces the desired pattern of responses. And these, after all, are the tests that the simulator is interested in demonstrating that his or her program can pass. Reviews and examples of the kinds of inferential tests that can be performed in the analysis of human-subject data collected via the information-processing approach can be found in Sternberg (1977, 1978, 1980b).

Second, information-processing techniques such as the subtraction method and the additive-factor method (see Pachella, 1974, for a readable description of these two methods) are highly useful in identifying processes that contribute to intelligent performance. Indeed, these techniques were formulated primarily with this goal in mind. It is often possible not only to identify these processes, but to identify as well the latencies or difficulties of the processes, the strategies into which the processes combine, the representations upon which the processes act, and the consistency over time with which the processes are used. Examples of such analyses can be found in the work of Hunt (Hunt, Frost, & Lunneborg, 1973; Hunt, Lunneborg, & Lewis, 1975), Pellegrino and Glaser (1980), R. Sternberg (1977, 1980b), S. Sternberg (1969), and many others.

Third, I do not see any indications that we have exhausted the potential of the information-processing paradigm to yield new insights into the nature of intelligence, nor do I see indications from the literature that others see this as an impending problem either. To the contrary, it has only been during the past decade that information-processing analyses of intelligent behavior have made a serious and concerted start (with a few earlier exceptions; see, e.g., Gagné, 1967), and all indications seem to be that it will be quite a while before the paradigm is exhausted in its

ability to yield new and interesting findings. Obviously, there is no "acid test" of what constitutes a still-productive paradigm. But the sheer volume of work being done in a large and growing number of different laboratories would seem to attest to the productivity of the paradigm in generating what many investigators apparently believe to be worthwhile research. In contrast, by the late nineteen sixties, the proportion of researchers still doing factorial analyses of intelligence had dwindled to a low level, after a prolonged period of slow but steady decline. Some of the interesting theoretical pursuits currently in progress include attempts to account for intelligent information processing in a variety of tasks via a relatively small number of information-processing components (e.g., Carroll, 1976; Hunt, 1978; Jensen, 1979; Pellegrino & Glaser, 1980; Sternberg, 1979); attempts to build computer models that can perform a wide variety of tasks intelligently (e.g., Anderson, 1976; Schank & Abelson, 1977; Simon & Lea, 1974); and attempts to account for mental retardation in information-processing terms (e.g., Butterfield & Belmont, 1977; Campione & Brown, 1978).

Fourth and finally, information-processing accounts of intelligent performance seem to provide implications for and be conducive to training because they decompose tasks into component processes that seem to be (at least in some cases) of a level of complexity that is compatible with instructional attempts. Training of intelligent performance has leaned heavily and directly upon the information-processing theories from which the training has been derived, and has been done successfully in a number of domains, including learning (e.g., Belmont & Butterfield, 1971; Campione & Brown, 1978), reasoning (e.g., Feuerstein, 1979; Holzman, Glaser, & Pellegrino, 1976; Sternberg & Weil, 1980), number skills (e.g., Resnick & Ford, in press), and problem solving (e.g., Siegler, 1978).

To conclude this section, on one view, information-processing analyses have been highly successful in meeting challenges that factorial methods have met with only partial success. But there is at least one other point of view.

A Pessimistic View

There is another side to the information-processing story, one that may lead to a more pessimistic assessment of the current state of the information-processing paradigm for studying intelligence. The pessimistic view is not inconsistent with the optimistic one: It is possible to be both optimistic and pessimistic simultaneously, depending upon the level of analysis one conducts. Consider once again the "four challenges."

First, although specific information-processing theories are usually disconfirmable (see, e.g., Sternberg, 1977, 1980b), the various subparadigms that generate these theories are not disconfirmable; moreover, they have generally been posed in ways that make it very hard to assess whether they are succeeding or failing. Consider some of the major information-processing subparadigms currently being used, and why I am afraid they can be "assured" of continued success.[2]

One subparadigm is what Pellegrino and Glaser (1979) have referred to as the cognitive-correlates approach. In this paradigm, introduced by Hunt, Frost, and Lunneborg (1973), parameters from rather simple information-processing tasks of the kind used in the cognitive psychologist's laboratory, e.g., the S. Sternberg (1969) memory-scanning task and the Posner-Mitchell (1967) letter-comparison task, are correlated with scores from standardized psychometric tests of mental ability. Hunt et al. computed these correlations, and found them generally to be at the level of what Mischel (1968) has called "personality coefficients": most of

the correlations are at the .30 level. How does one interpret correlations of this order of magnitude? Mischel (1968), noting the fact that almost anything in the personality literature correlated .30 with anything else, took a rather dim view of the progress that had been made toward understanding the nature of personality. He pointed to the literature on intellectual abilities as a bright spot, however, because correlations in that literature were generally higher. But Hunt and his colleagues did not share Mischel's chagrin, perhaps because researchers in the field of intelligence have not had to confront as directly as have researchers in the field of personality what seems to be a basic fact--that within a given domain such as personality or ability measurement, most concurrent test scores correlate at about the .30 level with each other. Hunt and his colleagues were sanguine, and it is worthwhile to quote them at length:

> The general psychologist will have noted that while we have shown significant results, we have not shown strong correlations. Indeed, our initial studies can be criticized on the grounds that we have shown a large number of moderate correlations, rather than having elucidated any one relationship in greater detail. Our data, then, may be more suggestive than conclusive. One of our original points, however, was that low positive correlations are of interest. Although it seems reasonable on a priori grounds to expect that the study of individual differences in cognition would locate the same critical underlying variables of human performance that would be revealed by an experimental approach to cognition, this, in fact, has not happened. Psychometrics and cognitive psychology appear to be currently sharing information about man, but by no means do they duplicate each other. At least, we see no other way to account for the pattern of our results. (pp. 114-115)

Levels of correlations between cognitive tasks of the kind studied by Hunt and his colleagues and psychometric tests were at similar levels in subsequent studies (see, e.g., Hunt, Lunneborg, & Lewis, 1975), although Hunt (1978) showed that with much wider ranges of ability levels (e.g., studies including retarded individuals), it is possible to boost these correlations, as would be expected from the increase in individual-difference variation caused by the expansion of range. The question one must pose regarding the "personality coefficients" Hunt has obtained in the majority of his studies is whether there is any set of plausible results that would differ from those Hunt obtained. On the one hand, it seems unlikely that the relatively low-level processing required by simple tasks such as comparing the physical appearance or names of letters would yield high correlations with relatively high-level tasks like complex algebra word problems. Indeed, a long-standing literature comparing the approaches of Galton (low-level tasks) and Binet (high-level tasks) prepared us for the extreme unlikelihood of such an event. On the other hand, it seems unlikely that the cognitive tasks and psycometric tests would be wholly uncorrelated or only trivially correlated. Complex problems such as algebra word problems or verbal analogies must draw at least to some extent on the perceptual and memory processes that are salient in the lower-level tasks. Although these processes are of much less importance in the higher-level tasks, they cannot be bypassed altogether, and to the extent that they are sources of individual differences, they will result in at least some correlation between the cognitive tasks and the psychometric tests.

To conclude our discussion of the cognitive-correlates approach, it is not clear what set of plausible outcomes would be inconsistent with the

utility of the cognitive-correlates approach, at least given Hunt, Frost, and Lunneborg's view of what constitutes success of the approach: Correlations that can be interpreted as "moderate" include a very wide range of values, and moreover, most cognitive measures are correlated at a "moderate" level with each other. These moderate correlations tell us nothing about directions of causal relationships, although one might interpret Hunt et al. (1975) as inferring from such correlations that speed of access to codes in working memory is to some extent causative of individual differences in verbal ability. Equally consistent with these data would be the possibility that high verbal ability leads to quick access, or that both high verbal ability and quick access are dependent upon some third variable.

Suppose high rather than moderate correlations are obtained in a cognitive-correlates study. What can one then conclude? Unless there is a strong theory underlying the newly discovered relationship, probably not much. I have seen numerous cases in which the high correlations represent nothing more than the fact that the predictors and criterion or criteria are very highly similar to each other, sometimes even on their face.

A second subparadigm is what Pellegrino and Glaser (1979) have called the cognitive-components approach. In this approach, investigators analyze complex tasks such as those actually found on intelligence tests, seeking to decompose performance on these tasks into elementary constituents of some kind, usually component processes. In this approach, the goal is to account for as much of the variation between stimulus types as possible, usually using either response time, response choice, or error rate as a dependent variable. Numerous tests of model fit can be performed (see Sternberg, 1978), so that one does not run into the problem of trying to decide what "moderate" level of fit to be happy with. Ideally, one wants as high a degree of model fit as is possible, given the reliability of the data.

In this subparadigm, disconfirmation of specific models of information processing is both possible and likely: Very few, if any, models are "true" in the sense that they specify veridically just what subjects do. Moreover, it is possible to reject alternative models, and to select the best one on a tentative basis. Although it may not be a "true" model, one can accept it as the best of the available models until a better model is found. But the experimental and quantitative rigor of the method hide what I believe to be cause for at least some concern. The approach in itself is not much more disconfirmable than is the cognitive-correlates approach: It, too, cannot fail if used to full advantage.

The cognitive-components approach requires more prior conceptualization and quantitative sophistication for its use than does the cognitive-correlates approach. The investigator must go in with a prior information-processing model that he or she has quantified or simulated on a computer, and can thus test for its validity. But if the investigator is able to do these things, then past experience indicates that there is almost always some componential model that will provide a very good fit to the data. Indeed, there is usually a linear model that will do so, since the predictions of linear models usually accord well with the predictions of nonlinear ones. Thus, given sufficient cleverness on the part of the investigator in manipulating independent variables that are sources of solution difficulty, and in formulating a model for how these sources combine to yield the total difficulty of the item type being studied, past experience suggests that the investigator is highly likely to succeed in modeling task performance. Of course, these are big "givens," and there may be many tasks that do not seem susceptible to this kind of modeling. But there seem to be enough tasks that are susceptible to this kind of modeling to keep investigators busy for quite a while. With enough parameters, of course, success

can be guaranteed, but previous modeling attempts in the literature suggest that most researchers can do quite well even with a fairly modest number of parameters. Even if the model cannot be rejected relative to the true model, one has no guarantee that the preferred model is the true model, since alternative models may all predict a given set of data with little or no departure of observed from predicted values (see, e.g., Carpenter & Just, 1975; Clark & Chase, 1972).

A third subparadigm is what might be called a training approach. In this approach, one starts

> with a detailed task analysis of a cognitive endeavor of particular interest to the theorist. If the analysis is thorough enough, it should be possible to instruct an immature learner (or a computer) to perform well on that task. If the instruction (or program) does not result in an appropriate type or level of performance, one likely cause is that the theory is not specified in sufficient detail. Ideally, the way in which performance deviates from optimality will provide more specific hints about the ways in which the theory needs elaboration.... Finally, the point where training ceases to be effective is of central importance to the development of a theory of intelligence. The underlying assumption is that as the difficulty of instructing some important component increases, i.e., the component begins to appear impervious to training, we would argue that the centrality of that component to intelligence also increases. (Campione, Brown, & Ferrara, in press)

This approach, which has been used by Campione and Brown (1978), Belmont and Butterfield (1971), and others, can be helpful in telling us what aspects of cognitive functioning are trainable with reasonable amounts of effort, and what kinds of functioning resist training. It can also be helpful sheerly at a practical level in improving people's cognitive

performance. But as an approach to helping us find out the nature of intelligence, I am concerned that it, like the other approaches, cannot fail: Failure in training does not really help us much in disconfirming a theory of intelligence. Unfortunately, even success in training is at best ambiguous in its interpretation.

Suppose, first, that one is unable to train subjects in a given sample of a population either to improve their proficiency in executing a postulated component of a theory of intelligence, or that one is unable to train them to use the componentat all. What can one conclude? There seem to be at least four highly plausible alternative interpretations of this outcome. The first is that the component is simply not a component of intelligence--that one cannot train it because it is not a natural part of a functioning cognitive system. A second interpretation is that the component is an aspect of intelligence, but that it is what Campione, Brown, and Ferrara refer to as "impervious to training." Not all intelligent acts need be accessible or even available to consciousness. For example, part of intelligent functioning is the generation of words to represent one's thoughts, although it is difficult to imagine how one could train a skill like this that is so inaccessible to consciousness. A third interpretation is that the component is an aspect of intelligence and is in fact trainable, but that the training methods used are inadequate. One can never know for sure that a failure to train subjects successfully is nothing more than a reflection of one's failure to devise training procedures that work. A fourth interpretation is that the component is an aspect of intelligence and that it is trainable, but not in the population being used in a given study. For example, Campione, Brown,

Belmont, Butterfield, and others have done most of their training work in populations of mentally retarded subjects. Could one reasonably conclude that failure to train a component process in such a population is an indication that the component is not an aspect of intelligence? One might equally well argue that its untrainability in such a population shows that the component is indeed an aspect of intelligence. To summarize, the interpretation of a failure in training is quite equivocal.

Suppose instead that one is able to train subjects in a given sample of a population either to improve their execution of a component or to execute the component at all. What, then, can one conclude? Unfortunately, not much. First, consider one interpretation of a component's being "impervious to training." In the passage from Campione, Brown, and Ferrara cited earlier, it is stated that "as the difficulty of instructing some important component increases,...the centrality of that component to intelligence also increases." But this statement implies that the more successful one is in training a component of intelligence, the less successful one is in demonstrating the centrality of that component to one's theory of intelligence, and vice versa. In other words, to succeed (in training) is to fail (in demonstrating theoretical importance of the component). If there was ever a prototypical example of nothing failing like success, certainly this is it! Second, sometimes subjects can be taught to perform tasks in ways that they almost never would perform them spontaneously; other times, they cannot be trained to perform the tasks in ways that they would have no trouble in using spontaneously. In linear syllogistic reasoning, for example, one can rather easily train subjects to use an algorithmic model that they would almost never use spontaneously; but it is extremely difficult to train them to use the mixture strategy that most subjects use

spontaneously (Sternberg & Weil, 1980). Would one want to claim that success in training the algorithmic model but not the mixed model indicates that the mixed model is not as good a source of information about the nature of intelligence, or vice versa? Certainly not, because in training strategies for analogical reasoning, the opposite pattern holds: It is quite easy to train the strategy subjects use spontaneously, but quite difficult to train the strategies subjects do not normally use (Sternberg & Ketron, Note 1; see also Sternberg, Ketron, & Powell, in press). One can only conclude that the trainability of a set of components (a strategy) bears no clear relation to the nature of intelligence. Third, even if one does succeed in training a component or strategy, one has no guarantee that the component or strategy represents part of what one should call _intelligent_ performance. The fact that a component can (or cannot) be trained does not in itself indicate whether that component is a part of intelligence as opposed to, for example, lower-level perception. The identification of the component as one aspect of intelligence must come from elsewhere (prior theory, correlations with other measures, or whatever), which brings one back to the question that one started with, namely, how does one identify the components that constitute aspects of intelligent performance?

I have reviewed three of the major information-processing subparadigms for understanding the nature of intelligence, and have concluded that in at least one respect, they face the same difficulty as factor analysis. The difficulty is better disguised, but may be viewed as all the more pernicious because of its unobviousness. The problem is that in at least one sense, each subparadigm cannot but succeed in meeting the goals its users and even many of its critics have set for it, and hence, in at least one sense, it cannot fail. I will claim next that information-

processing approaches share other problems of factor analysis, although, again, in an unobvious way. Consider now the "second" challenge.

Second, there can be no doubt that information-processing approaches have fulfilled their promise of identifying component processes in intelligent performance (see, e.g., Hunt, 1978; Mulholland, Pellegrino, & Glaser, 1980; Sternberg, 1977, 1980c). But certain problems lurk beneath the surface: Our success no longer seems as "successful" as we had once hoped it would seem.

One goal of information-processing analysis was to identify a unit of analysis that would in some sense be basic--a unit, for example, in terms of which individual differences in factor scores could be understood (Carroll, 1976; Sternberg, 1977). The hope was that the component process would serve as such a unit, and ultimately provide a "common currency" for the exchange of views regarding the basic nature of intelligence. But it has become increasingly clear that we really have no way of determining constitutes a basic unit, nor are we, I suspect, even clear as to just what we mean by a basic unit. For example, we have no way at the present time of knowing whether the factor or the component is the more basic unit. People seem to take all possible positions. Carroll (1980) now seems to claim that the factor is the more basic unit, and that it is responsible for individual differences in components; I previously claimed that the component was the more basic unit, and that it was responsible for generating individual differences in factors (Sternberg, 1977). I now believe that the question is not a meaningful one in our present state of knowledge (Sternberg, 1980a). We are no better able to say which unit is more basic than we are able to say which came first, the chicken or the egg. Factor scores can be regressed on component scores; component scores can be regressed on factor scores. What experimental or mathematical operation would enable us to claim with

any confidence that one unit is more basic than the other?

A second goal of information-processing analysis was to tell us how subjects solve complex problems requiring intelligent performance. At one level, information-processing analysis has told us that. We can say, at least to some order of approximation, that solution of analogies requires the execution of operations such as encoding, inference, application, and the like, or that solution of linear syllogisms requires execution of operations such as premise reading, processing of marked adjectives, combination of terms into a visualized spatial array, and the like (see Sternberg, 1977, 1980b). But as Pellegrino and Lyon (1979), among others, have pointed out, the components identified in my and others' "componential" analyses are black boxes. Some of the information we would be most interested in would come from our figuring out what mental events occur during encoding, inference, premise reading, and so on. In other words, how does a person do these things? One approach to this problem is to decompose information-processing performance into smaller components or subcomponents than the ones we have used. Such an approach at least reduces the magnitude of the problem. To date, we have no evidence that it is capable of eliminating the problem altogether.

A third goal of information-processing analysis was to tell us what intelligent performance consists of. But we need to ask ourselves whether the processes we are identifying are ones that we can confidently identify as sources of individual differences in interesting kinds of intelligent performance that occur in the real world, such as making a career choice,

performing well in one's career, deciding how to schedule one's time to maximize one's operating efficiency, and so on. I have serious doubts that the kinds of processes being identified in cognitive-correlate analysis are on the right track. Speed of naming two letters as "same" or "different" does seem to me quite removed from ordinary conceptions of intelligent performance or its antecedents. I have more confidence that the kinds of processes being identified in cognitive-component analysis are on the right track. For example, I am prepared to believe that "inference" is an integral part of intelligent functioning in the real world. I am much less ready to believe, however, that "inference" of the kind used in solving analogy test items is the same as, or closely related to, say, inference in seeing relations between two important historical events, or between two economic indicators. We have what I perceive to be a "levels of processing" problem. There is a large gap between the levels of inference used in laboratory or psychometric tasks and the levels used in more consequential kinds of reasoning performance. And the difference in levels may be of a qualitative as well as of a quantitative nature. Although there have been reasonably successful attempts to show that the parameters named in the same way across different laboratory information-processing tasks are correspondent (Chiang & Atkinson, 1976; Sternberg & Gardner,in press);there have been no attempts to relate these parameters to performance in real-world performance.

I have claimed that although information-processing analysis has identified processes of task performance, this identification has not proven to be the panacea many people hoped it would be. Many of the questions we would like answered about component processes still remain. And other problems remain for the information-processing approach as well.

Third, on the face of things, the information-processing paradigm would certainly not seem to have been exhausted. But we must ask ourselves exactly what it means for a paradigm to be exhausted. Is it, for example, conventional factor analysis that has been (in the opinion of some, at least) exhausted in the factorial paradigm, or is it the types of uses to which we have put conventional factor analysis? I suspect that the latter is the case. We pretty much ran out of tasks to factor analyze or that we cared to factor analyze, and it wasn't clear where to go from where we were with factor analysis. The critical question concerns not so much the technique itself as the use to which the technique is to be put to continue to yield interesting new information about the nature of intelligence.

We have not yet run out of tasks upon which to conduct information-processing analyses. We have yet to see (convincing) process models of anagram performance, remote associates performance, counting of cubes from a three-dimensional surface represented in two dimensions, etc. The question we must ask, though, is that of what is to be gained from isolating component processes from still more tasks like the ones we have analyzed? A number of studies have been done in which component process scores from cognitive tasks have been correlated with scores from psychometric ability tests (e.g., Hunt et al., 1973, 1975; Sternberg, 1977, 1980_b). As noted earlier, the correlations between scores on these tasks and scores on psychometric ability tests have been less than impressive. But these psychometric tests have only been proxies for the criteria we really do or at least should care about--namely, performances in real-world tasks. Although cognitive process scores have not, to my knowledge,

been correlated with performance such as school grades, supervisory or peer ratings, income, or whatever "real-world" criteria one would like, I am inclined to believe that almost inevitably, these correlations would be lower than those that have been obtained with psychometric tests. Such reduced correlations would stem not only from the reduced reliabilities of the criterion measures, I believe, but also from the reduced complexity of the cognitive components relative to the composite psychometric tests that have until now served as the predictors of real-world performance. Past experience in research on intelligence has shown to almost everyone's satisfaction that higher predictive validities for complex outcomes are almost always associated with greater complexities in predictors. Indeed, increasing the reliability of a predictor by simplifying its structure often results in a decrease rather than an increase in predictive validity. We must therefore ask ourselves whether still more information-processing analyses of the kinds of tasks we have studied are likely to turn things around. I suspect they are not. We need new kinds of tasks.

Fourth, consider again the issue of training component information processes. There can be little doubt that some training of cognitive processes is both possible and feasible (see, e.g., Borkowski & Cavanaugh, 1979; Feuerstein, 1979; Holzman, Glaser, & Pellegrino, 1976). Although evidence supporting the durability and generalizability of such training is still meager, we have cause to be at least modestly optimistic regarding the feasibility of training some cognitive skills (Brown & Campione, in press). I believe it important that these training efforts continue, because in terms of theoretically-based programs, training of cognitive skills is pretty much our best option now, even if at times these cognitive skills are more narrow than ideally we'd like. Eventually, though, I think

it important that we supplement these training programs with training in real-world problem solving and decision making of the kinds needed for important events in one's life. Ultimately, the real-world behaviors rather than their proxies are what we are interested in, and we can at least hope that any training effects we can get through the proxies will be strengthened by direct training of the real-world behaviors that we hope to affect.

Conclusions

I have argued that the present state of research on intelligence could be conceived of as on the borderline of a crisis period: Conventional factor-analytic research on intelligence has been less than successful in meeting four challenges that confront intelligence research; although on the surface, information-processing research has been quite a bit more successful in meeting these challenges, at a deeper level, these approaches, too, have been less successful than one might wish. What conclusions can be drawn from the review? I believe there are at least three reasonable ones.

First, we should be wary of a trend in intelligence research to reject old approaches (or new competitive ones) to studying intelligence in favor of our own preferred ones. In the initial burst of enthusiasm that accompanies the success of a new methodology, there is an understandable tendency to view the new methodology as a panacea for the problems of old or new competitive methodologies. There is also a tendency to attempt to sell a new method not only on its virtues, but on the alleged limitations of its competitors. Thus, we have been treated to disquisitions on why cognitive-components analysis is to be preferred over factor analysis (Sternberg, 1977) or cognitive-correlates analysis

(Pellegrino & Glaser, 1979); on why analysis of covariance structures (Frederiksen, in press) or latent trait analysis (Whitely, 1980) is to be preferred over cognitive-components analysis; or on why, if one's goal is to isolate "latent abilities," traditional factor analysis is to be preferred over these and other alternatives (Carroll, in press; Guilford, in press). I am inclined to believe, however, that all of the methods now available have overlapping strengths and weaknesses. The best strategy to follow is to attempt to show in what respects different methodologies lead to the same conclusions in some respects and different conclusions in others regarding the nature of intelligence. I also believe that various methods in fact show striking convergences in the generalizations to which they lead us about the nature of intelligence (Sternberg, 1980a, in press-b, in press-c), a belief I shall discuss further shortly.

Second, I think we need to think more about the criteria we wish to use in evaluating the relative successes of various approaches to studying intelligence. One could draw the conclusion from this review--wrongly, I believe--that none of the methods are very useful because they are flawed in so many respects. A more valid conclusion, I believe, would be that probably all of the methods that have been used can lead to important insights or to dull ones. Advocates of one approach can often turn around the arguments being leveled against their approach by advocates of another approach to apply to that other approach. The value of a contribution seems to lie in how creatively and insightfully a given method is used by an investigator, rather than in the method itself. What seems to matter most is not what method is used, but how it is used. Attempts to argue for or against methods, in the abstract, seem not to be terribly fruitful.

A better use of time might be in arguing about ways in which one or more methods might be put to more productive use by psychologists interested in using the method, in effect, to think of method-investigator interactions. If no better use can be found for a method at a given time, it should by all means be put into cold storage until, perhaps at some later time, a better use is thought of for it.

Third, we need to remember the oft-repeated admonition that the validity of a theory can be adequately tested only through the use of converging operations (Garner, Hake, & Eriksen, 1956). Any one method for studying a psychological phenomenon is incomplete in some respects, and inadequate in others. But if the same phenomenon appears almost without regard to the method that is used to uncover or analyze it, then one's confidence in the validity of the phenomenon is increased. Although each of the methods I have reviewed has inadequacies, the use of a combination of methods (including, of course, ones not reviewed here) can provide a powerful demonstration of a phenomenon of interest.

Finally, I think we need to consider more carefully our choice of tasks in studying intelligent behavior. A common theme running throughout this article has been that in studying sometimes remote proxies for interesting real-world behaviors, there has been some loss in the real-world significance of the outcomes. I would not take the position of Cole (Note 2) that the results of these studies have been misleading. Rather, they have been incomplete. I would like to see our laboratory studies of intelligence supplemented with (but by no means replaced by) studies of real-world behaviors or simulations of such behaviors. The behaviors studied should be consequential ones, such as choosing a college, a career, or a mate, or deciding whether or not to pursue major surgery. Such work has already been undertaken by several investigators. Frederiksen (1962, 1966; Frederiksen, Saunders, & Wand, 1957) has been a pioneer in this respect in his

direction-setting studies of the in-basket technique and in his investigations of creative hypothesis formation and evaluation in scientific thinking (Frederiksen & Evans, 1974). Hayes-Roth and Hayes-Roth (1979) have proposed a theory of real-world kinds of planning, such as the order in which one plans to carry out a sequence of daily errands, and Goldin and Hayes-Roth (Note 3) have found systematic individual differences in the nature of the planning process. Our own recent research has begun to take a more practical bent as well. Rick Wagner and I are investigating the kinds of practical skills and knowledge of value systems people in everyday life and in professions such as law and psychology need to get ahead in their respective pursuits, e.g., how people decide what activities are worth doing in limited amounts of time, and how they budget their time according to the value they place on each activity. Craig Smith and I are investigating the construct of social intelligence, following in the footsteps of Archer (1980) and Rosenthal, Hall, DiMatteo, Rogers, and Archer (1979), among others, in examining how people decode implicit communication, such as the nonverbal cues people emit in expressing approval or disapproval. We are interested in isolating components of social intelligence, if they exist, and in relating them to each other and to components of cognitive intelligence.

Laboratory research has been, and I believe, will continue to be, useful in isolating various aspects of intelligent performance. A recent and particularly promising development has been in the investigation of knowledge representations in people of various levels of expertise who are engaged in solving complex problems such as those found in physics (Larkin, McDermott, Simon, & Simon, 1980; Chi, Feltovich, & Glaser, Note 4) and geometry (Greeno, 1976). My research and that of others (e.g., Hogaboam & Pellegrino, 1978) leads me to believe that a particularly promising route to pursue in the study of laboratory tasks will be that of what I have called *nonentrenched*, i.e., novel, tasks (Sternberg, in press).

Performance on such tasks seems to be substantially more highly correlated with performance on psychometric tests than performance on more standard laboratory tasks is correlated with performance on the tests. I agree with Cole (Note 3) and Neisser (1976), however, that we need to pay more attention to macroscopic aspects of information processing that are sometimes overlooked in laboratory task analysis. At the present time, our knowledge of high-level performance in real-world tasks is meager. But if our goal in research on intelligence is to understand intelligence as successful adaptation to and purposive action in one's real-world environment, knowledge about such relations would seem to be essential.

Reference Notes

1. Sternberg, R. J., & Ketron, J. L. *Process and strategy training of analogical reasoning*. Manuscript in preparation, 1980.
2. Cole, M. *Niche-picking*. Unpublished manuscript, 1980.
3. Goldin, S. E., & Hayes-Roth, B. *Individual differences in planning processes*. (Office of Naval Research Technical Report.) Santa Monica, California: Rand Corporation, July, 1980.
4. Chi, M. T. H., Feltovich, P. J., & Glaser, R. *Representation of physics knowledge by experts and novices*. (NR 157-421 ONR Technical Report No. 2.) Pittsburgh: Learning, Research, and Development Center, University of Pittsburgh, March, 1980.

References

Anderson, J. R. *Language, memory, and thought*. Hillsdale, N.J.: Erlbaum, 1976.

Archer, D. *How to expand your social intelligence quotient*. New York: M. Evans, 1980.

Belmont, J. M., & Butterfield, E. C. Learning strategies as determinants of memory deficiencies. *Cognitive Psychology*, 1971, *2*, 411-420.

Borkowski, J. G., & Cavanaugh, J. C. Maintenance and generalization of skills and strategies by the retarded. In N. R. Ellis (Ed.), *Handbook of mental deficiency* (second edition). Hillsdale, N.J.: Erlbaum, 1979.

Brown, A. L., & Campione, J. C. How, and how much, can intelligence be modified? *Intelligence*, in press.

Burt, C. The genetics of intelligence. In W. B. Dockrell (Ed.), *On intelligence*. Toronto: Ontario Institute for Studies in Education, 1970.

Butterfield, E. C., & Belmont, J. M. Assessing and improving the executive cognitive functions of mentally retarded people. In I. Bialer & M. Sternlicht (Eds.), *Psychological issues in mental retardation*. New York: Psychological Dimensions, 1977.

Campione, J. C., & Brown, A. L. Toward a theory of intelligence: Contributions from research with retarded children. *Intelligence*, 1978, 2, 279-304.

Campione, J. C., Brown, A. L., & Ferrara, R. Research with slow-learning children: Implications for the concept of intelligence. In R. J. Sternberg (Ed.), *Handbook of human intelligence*. New York: Cambridge University Press, in press.

Carpenter, P. A., & Just, M. A. Sentence comprehension: A psycholinguistic processing model of verification. *Psychological Review*, 1975, 82, 45-73.

Carroll, J. B. Psychometric tests as cognitive tasks: A "new structure of intellect." In L. B. Resnick (Ed.), *The nature of intelligence*. Hillsdale,

N.J.: Erlbaum, 1976.

Carroll, J. B. Remarks on R. J. Sternberg's "Factor theories of intelligence are all right almost." Educational Researcher, 1980, 9, 14-18.

Cattell, R. B. Abilities: Their structure, growth, and action. Boston: Houghton-Mifflin, 1971.

Chiang, A., & Atkinson, R. C. Individual differences and interrelationships among a select set of cognitive skills. Memory and Cognition, 1976, 4, 661-672.

Clark, H., & Chase, W. On the process of comparing sentences against pictures. Cognitive Psychology, 1972, 3, 472-517.

Feuerstein, R. The dynamic assessment of retarded performers: The learning potential assessment device, theory, instruments, and techniques. Baltimore: University Park Press, 1979.

Frederiksen, J. R. Component skills in reading: Measurement of individual differences through chronometric analysis. In R. E. Snow, P.-A. Federico, & W. Montague (Eds.), Aptitude, learning, and instruction: Cognitive process analysis. Hillsdale, N.J.: Erlbaum, 1980.

Frederiksen, J. R. A Thurstonian's reaction to a componential theory of intelligence. Behavioral and Brain Sciences, in press.

Frederiksen, N. Factors in in-basket performance. Psychological Monographs: General and Applied, 1962, 76, Whole No. 541.

Frederiksen, N. Validation of a simulation technique. Organizational Behavior and Human Performance, 1966, 1, 87-109.

Frederiksen, N., & Evans, F. R. Effects of models of creative performance on ability to formulate hypotheses. Journal of Educational Psychology, 1974, 66, 67-82.

Frederiksen, N., Saunders, D. R., & Wand, B. The in-basket test. Psychological Monographs: General and Applied, 1957, 71, Whole No. 438.

Gagnè, R. M. (Ed.), Learning and individual differences. Columbus, Ohio: Merrill, 1967.

Garner, W. R., Hake, H. W., & Eriksen, C. W. Operationism and the concept of perception. Psychological Review, 1956, 63, 149-159.

Greeno, J. G. Indefinite goals in well-structured problems. Psychological Review, 1976, 83, 479-491.

Guilford, J. P. The nature of human intelligence. New York: McGraw-Hill, 1967.

Guilford, J. P. Components versus factors. Behavioral and Brain Sciences, in press.

Hayes-Roth, B., & Hayes-Roth, F. A cognitive model of planning. Cognitive Science, 1979, 3, 275-310.

Hogaboam, T. W., & Pellegrino, J. W. Hunting for individual differences in cognitive processes: Verbal ability and semantic processing of pictures and words. Memory and Cognition, 1978, 6, 189-193.

Holzman, T. G., Glaser, R., & Pellegrino, J. W. Process training derived from a computer simulation theory. Memory and Cognition, 1976, 4, 349-356.

Horn, J. L. On subjectivity in factor analysis. Educational and Psychological Measurement, 1967, 27, 811-820.

Horn, J. L. Organization of abilities and the development of intelligence. Psychological Review, 1968, 75, 242-259.

Horn, J. L., & Knapp, J. R. On the subjective character of the empirical base of Guilford's structure-of-intellect model. Psychological Bulletin, 1973, 80, 33-43.

Humphreys, L. G. The organization of human abilities. American Psychologist, 1962, 17, 475-483.

Hunt, E. B. Mechanics of verbal ability. Psychological Review, 1978, 85, 109-130.

Hunt, E. B., Frost, N., & Lunneborg, C. Individual differences in cognition: A new approach to cognition. In G. Bower (Ed.), The psychology of learning and motivation (Vol. 7). New York: Academic Press, 1973.

Hunt, E., Lunneborg, C., & Lewis, J. What does it mean to be high verbal? Cognitive Psychology, 1975, 7, 194-227.

Jensen, A. R. g: Outmoded theory of unconquered frontier? Creative Science and Technology, 1979, 2, 16-29.

Jöreskog, K. G. A general method for analysis of covariance structures. Biometrika, 1970, 57, 239-251.

Larkin, J., McDermott, J., Simon, D., & Simon, H. Expert and novice performance in solving physics problems. Science, 1980, 208, 1335-1342.

Mischel, W. Personality and assessment. New York: Wiley, 1968.

Mulholland, T. M., Pellegrino, J. W., & Glaser, R. Components of geometric analogy solution. Cognitive Psychology, 1980, 12, 252-284.

Neisser, U. General, academic, and artificial intelligence. In L. B. Resnick (Ed.), The nature of intelligence. Hillsdale, N.J.: Erlbaum, 1976.

Pachella, R. G. The interpretation of reaction time in information processing research. In B. Kantowitz (Ed.), Human information processing: Tutorials in performance and cognition. Hillsdale, N.J.: Erlbaum, 1974.

Pellegrino, J. W., & Glaser, R. Cognitive correlates and components in the analysis of individual differences. In R. J. Sternberg & D. K. Detterman (Eds.), Human intelligence: Perspectives on its theory and measurement. Norwood, N.J.: Ablex, 1979.

Pellegrino, J. W., & Glaser, R. Components of inductive reasoning. In R. E. Snow, P.-A. Federico, & W. Montague (Eds.), Aptitude, learning, and instruction: Cognitive process analysis (Vol. 1). Hillsdale, N.J.: Erlbaum, 1980.

Pellegrino, J. W., & Lyon, D. R. The components of a componential analysis. Intelligence, 1979, 3, 169-186.

Posner, M. I., & Mitchell, R. Chronometric analysis of classification. Psychological Review, 1967, 74, 392-409.

Resnick, L. B., & Ford, W. W. The psychology of mathematics for instruction. Hillsdale, N.J.: Erlbaum, in press.

Rosenthal, R., Hall, J., DiMatteo, M. R., Rogers, P. L., & Archer, D. Sensitivity to nonverbal communication: The PONS Test. Baltimore: Johns Hopkins Press, 1979.

Schank, R., & Abelson, R. Scripts, plans, goals, and understanding. Hillsdale, N.J.: Erlbaum, 1977.

Siegler, R. S. The origins of scientific reasoning. In R. S. Siegler (Ed.), Children's thinking: What develops? Hillsdale, N.J.: Erlbaum, 1978.

Simon, H. A., & Lea, G. Problem solving and rule induction: A unified view. In L. W. Gregg (Ed.), Knowledge and cognition. Hillsdale, N.J.: Erlbaum, 1974.

Snow, R. E. Theory and method for research on aptitude processes. Intelligence, 1978, 2, 225-278.

Spearman, C. 'General intelligence,' objectively determined and measured. American Journal of Psychology, 1904, 15, 201-293.

Sternberg, R. J. Intelligence, information processing, and analogical reasoning: The componential analysis of human abilities. Hillsdale, N.J.: Erlbaum, 1977.

Sternberg, R. J. Intelligence research at the interface between differential and cognitive psychology. Intelligence, 1978, 2, 195-222.

Sternberg, R. J. The nature of mental abilities. American Psychologist, 1979, 34, 214-230.

Sternberg, R. J. Factor theories of intelligence are all right almost. Educational Researcher, 1980, 9, 6-13, 18. (a)

Sternberg, R. J. Representation and process in linear syllogistic reasoning.

Journal of Experimental Psychology: General, 1980, 109, 119-159.

Sternberg, R. J. Intelligence and nonentrenchment. Journal of Educational Psychology, in press. (a)

Sternberg, R. J. The nature of intelligence. New York University Education Quarterly, in press. (b)

Sternberg, R. J. Sketch of a componential subtheory of human intelligence. Behavioral and Brain Sciences, in press. (c)

Sternberg, R. J., Gardner, M. K. A componential interpretation of the general factor in human intelligence. In H. Eysenck (Ed.), A model for intelligence. Berlin: Springer, in press.

Sternberg, R. J., Ketron, J. L., & Powell, J. S. Componential approaches to the training of intelligence. Intelligence, in press.

Sternberg, R. J., & Weil, E. M. An aptitude-strategy interaction in linear syllogistic reasoning. Journal of Educational Psychology, 1980, 72, 226-23[illegible].

Sternberg, S. Memory-scanning: Mental processes revealed by reaction-time experiments. American Scientist, 1969, 4, 421-457.

Thurstone, L. L. Primary mental abilities. Chicago: University of Chicago Press, 1938.

Vernon, P. E. The structure of human abilities. London: Methuen, 1971.

Whitely, S. E. Latent trait models in the study of intelligence. Intelligence, 1980, 4, 97-132.

Footnotes

Preparation of this article was supported by Contract N0001478C0025 from the Office of Naval Research to Robert J. Sternberg. I am grateful to Janet Powell for comments on an earlier version of the manuscript.
Requests for reprints should be sent to Robert J. Sternberg, Department of Psychology, Yale University, Box 11A Yale Station, New Haven, Connecticut 06520.

[1] By nonconfirmatory methods, I mean those methods of factor analysis that do not use maximum-likelihood estimation procedures to test the validity of a prior structural model. Confirmatory methods have come into widespread use only during the past decade or so (see, e.g., Jöreskog, 1970). These methods, I believe, can be highly useful in hypothesis testing.

[2] The distinguishability of alternative subparadigms is often hazy, especially since it is possible to combine subparadigms within a single study. Hence, some of the criticisms directed at use of one particular paradigm may apply to another in certain instances, whereas others of the criticisms directed at that paradigm may not apply at all in certain instances.

Technical Reports Presently in this Series

NR 150-412, ONR Contract N0001478C0025

No.	Name	Published Reference
1	Intelligence Research at the Interface between Differential and Cognitive Psychology. January, 1978.	Sternberg, R. J. Intelligence research at the interface between differential and cognitive psychology. Intelligence, 1978, 2, 195-222.
2	Isolating the Components of Intelligence. January, 1978.	Sternberg, R. J. Isolating the components of intelligence. Intelligence, 1978, 2, 117-128.
3	Deductive Reasoning. January, 1978.	Sternberg, R. J., Guyote, M. J., & Turner, M. E. Deductive reasoning. In R. E. Snow, P.-A. Federico, & W. Montague (Eds.), Aptitude, learning and instruction: Cognitive process analysis (Vol. 1). Hillsdale, N.J.: Erlbaum, 1980.
4	Toward a Unified Componential Theory of Human Reasoning. April, 1978.	Sternberg, R. J. Toward a unified componential theory of human intelligence: I. Fluid ability. In M. Friedman, J. Das, & N. O'Connor (Eds.), Intelligence and learning. New York: Plenum, 1980.
5	A Transitive-Chain Theory of Syllogistic Reasoning. April, 1978.	UNPUBLISHED TO DATE
6	Components of Syllogistic Reasoning. April, 1978.	Sternberg, R. J., & Turner, M. E. Components of syllogistic reasoning. Acta Psychologica, in press.
7	Metaphor, Induction, and Social Policy: The Convergence of Macroscopic and Microscopic Views. April, 1978.	Sternberg, R. J., Tourangeau, R., & Nigro, G. Metaphor, induction, and social policy: The convergence of macroscopic and microscopic views. In A. Ortony (Ed.), Metaphor and thought. New York: Cambridge University Press, 1979.
8	A Proposed Resolution of Curious Conflicts in the Literature on Linear Syllogisms. June, 1978.	Sternberg, R. J. A proposed resolution of curious conflicts in the literature on linear syllogisms. In R. Nickerson (Ed.), Attention and performance VIII. Hillsdale, N.J.: Erlbaum, 1980.
9	The Nature of Mental Abilities. June, 1978.	Sternberg, R. J. The nature of mental abilities. American Psychologist, 1979, 34, 214-230.

Technical Reports Presently in this Series

NR 150-412

No.	Name	Published Reference
10	Psychometrics, Mathematical Psychology, and Cognition: Confessions of a Closet Psychometrician. June, 1978.	UNPUBLISHABLE.
11	Understanding and Appreciating Metaphors. June, 1978.	UNPUBLISHED TO DATE.
12.	Representation and Process in Transitive Inference. October, 1978.	Sternberg, R. J. Representation and process in linear syllogistic reasoning. Journal of Experimental Psychology: General, 1980, 109, 119-159.
13	Aptness in Metaphor. October, 1978.	Tourangeau, R., & Sternberg, R. J. Aptness in metaphor. Cognitive Psychology, in press.
14	Contrasting Conceptions of Intelligence and their Educational Implications. November, 1978.	Sternberg, R. J. Factor theories of intelligence are all right almost. Educational Researcher, in press.
15	An Aptitude-Strategy Interaction in Linear Syllogistic Reasoning. April, 1979.	Sternberg, R. J., & Weil, E. M. An aptitude-strategy interaction in linear syllogistic reasoning. Journal of Educational Psychology, 1980, 72, 226-234.
16	Intelligence Tests in the Year 2000: What Forms will they Take and what Purposes will they Serve? April, 1979.	Sternberg, R. J. Six authors in search of a character: A play about intelligence tests in the year 2000. Intelligence, 1979, 3, 281-291.
17	New Views on IQs: A Silent Revolution of the 70s. April, 1979.	Sternberg, R. J. Stalking the I.Q. quark. Psychology Today, 1979, 13, 42-54.
18	Unities in Inductive Reasoning. October, 1979.	UNPUBLISHED TO DATE.
19	Components of Human Intelligence. October, 1979.	Sternberg, R. J. Sketch of a componential subtheory of human intelligence. Behavioral and Brain Sciences, in press.
20	The Construct Validity of Aptitude Tests: An Information-Processing Assessment. October, 1979.	Sternberg, R. J. The construct validity of aptitude tests: An information-processing assessment. In

Technical Reports Presently in this Series

NR 150-412

Page 3

No.	Name	Published Reference
20 (Continued)		A. P. Maslow, R. H. McKillup, & M. Thatcher (Eds.), Construct validity in psychological measurement. Princeton: Educational Testing Service, in press.
21	Evaluation of Evidence in Causal Inference. October, 1979.	Schustack, M. W., & Sternberg, R. J. Evaluation of evidence in causal inference. Journal of Experimental Psychology: General, in press.
22	Componential Approaches to the Training of Intelligent Performance. April, 1980.	Sternberg, R. J., Ketron, J. L., & Powell, J S. Componential approaches to the training of intelligent performance. Intelligence, in press.
23	Intelligence and Nonentrenchment. April, 1980.	UNPUBLISHED TO DATE.
24	Reasoning, Problem Solving, and Intelligence. April, 1980.	Sternberg, R. J. Reasoning, problem solving, and intelligence. In R. J. Sternberg (Ed) Handbook of human intelligence. New York: Cambridge University Press, in press.
25	Claims, Counterclaims, and Components: A Countercritique of Componential Analysis. June, 1980.	Sternberg, R. J. Claims, counterclaims, and components: A countercritique of componential analysis. Behavioral and Brain Sciences, in press.
26	Interaction and Analogy in the Comprehension and Appreciation of Metaphors. October, 1980.	UNPUBLISHED TO DATE.
27	The Nature of Intelligence. October, 1980.	Sternberg, R. J. The nature of intelligence. New York University Education Quarterly, in press.
28	People's Conceptions of Intelligence. October, 1980.	Sternberg, R. J., Conway, B. E., Ketron, J. L., & Bernstein, M. People's conceptions of intelligence. Journal of Personality and Social Psychology: Attitudes and Social Cognition, in press.

Technical Reports Presently in this Series

NR 150-412, ONR Contract N0001478C0025

No.	Name	Published Reference
29	Nothing Fails Like Success: The Search for an Intelligent Paradigm for Studying Intelligence.	Sternberg, R. J. Nothing fails like success: The search for an intelligent paradigm for studying intelligence. Journal of Educational Psychology, in press.
30	Reasoning with Determinate and Indeterminate Linear Syllogisms.	NOT YET PUBLISHED.
31	A Componential Interpretation of the General Factor in Human Intelligence.	Sternberg, R. J., & Gardner, M. K. A componential interpretation of the general factor in human intelligence. In H. J. Eysenck (Ed.), A model for intelligence. Berlin: Springer, in press.

Navy

1 Dr. Ed Aiken
Navy Personnel R&D Center
San Diego, CA 92152

1 Meryl S. Baker
NPRDC
Code P309
San Diego, CA 92152

1 Dr. Jack R. Borsting
Provost & Academic Dean
U.S. Naval Postgraduate School
Monterey, CA 93940

1 Dr. Robert Breaux
Code N-711
NAVTRAEQUIPCEN
Orlando, FL 32813

1 Chief of Naval Education and Training
Liason Office
Air Force Human Resource Laboratory
Flying Training Division
WILLIAMS AFB, AZ 85224

1 Dr. Larry Dean, LT, MSC, USN
Psychology Department
Naval Submarine Medical Research Lab
Naval Submarine Base
Groton, CT 06340

1 Dr. Richard Elster
Department of Administrative Sciences
Naval Postgraduate School
Monterey, CA 93940

1 DR. PAT FEDERICO
NAVY PERSONNEL R&D CENTER
SAN DIEGO, CA 92152

1 Mr. Paul Foley
Navy Personnel R&D Center
San Diego, CA 92152

1 Dr. John Ford
Navy Personnel R&D Center
San Diego, CA 92152

Navy

1 Dr. Henry M. Halff
Department of Psychology, C-009
University of California at San Diego
La Jolla, CA 92093

1 LT Steven D. Harris, MSC, USN
Code 6021
Naval Air Development Center
Warminster, Pennsylvania 18974

1 Dr. Patrick R. Harrison
Psychology Course Director
LEADERSHIP & LAW DEPT. (7b)
DIV. OF PROFESSIONAL DEVELOPMMENT
U.S. NAVAL ACADEMY
ANNAPOLIS, MD 21402

1 Dr. Jim Hollan
Code 304
Navy Personnel R & D Center
San Diego, CA 92152

1 CDR Charles W. Hutchins
Naval Air Systems Command Hq
AIR-340F
Navy Department
Washington, DC 20361

1 CDR Robert S. Kennedy
Head, Human Performance Sciences
Naval Aerospace Medical Research Lab
Box 29407
New Orleans, LA 70189

1 Dr. Norman J. Kerr
Chief of Naval Technical Training
Naval Air Station Memphis (75)
Millington, TN 38054

1 Dr. William L. Maloy
Principal Civilian Advisor for
Education and Training
Naval Training Command, Code 00A
Pensacola, FL 32508

1 Dr. Kneale Marshall
Scientific Advisor to DCNO(MPT)
OP01T
Washington DC 20370

Navy

1 CAPT Richard L. Martin, USN
Prospective Commanding Officer
USS Carl Vinson (CVN-70)
Newport News Shipbuilding and Drydock Co
Newport News, VA 23607

1 Dr. James McBride
Navy Personnel R&D Center
San Diego, CA 92152

1 Dr. George Moeller
Head, Human Factors Dept.
Naval Submarine Medical Research Lab
Groton, CN 06340

1 Dr William Montague
Navy Personnel R&D Center
San Diego, CA 92152

1 Library
Naval Health Research Center
P. O. Box 85122
San Diego, CA 92138

1 Naval Medical R&D Command
Code 44
National Naval Medical Center
Bethesda, MD 20014

1 CAPT Paul Nelson, USN
Chief, Medical Service Corps
Bureau of Medicine & Surgery (MED-2[illegible])
U. S. Department of the Navy
Washington, DC 20372

1 Ted M. I. Yellen
Technical Information Office, Code [illegible]
NAVY PERSONNEL R&D CENTER
SAN DIEGO, CA 92152

1 Library, Code P201L
Navy Personnel R&D Center
San Diego, CA 92152

6 Commanding Officer
Naval Research Laboratory
Code 2627
Washington, DC 20390

Navy

1 Psychologist
ONR Branch Office
Bldg 114, Section D
666 Summer Street
Boston, MA 02210

1 Psychologist
ONR Branch Office
536 S. Clark Street
Chicago, IL 60605

1 Office of Naval Research
Code 437
800 N. Quincy SStreet
Arlington, VA 22217

1 Office of Naval Research
Code 441
800 N. Quincy Street
Arlington, VA 22217

5 Personnel & Training Research Progr[illegible]
(Code 458)
Office of Naval Research
Arlington, VA 22217

1 Psychologist
ONR Branch Office
1030 East Green Street
Pasadena, CA 91101

1 Office of the Chief of Naval Operati[illegible]
Research Development & Studies Bran[illegible]
(OP-115)
Washington, DC 20350

1 Dr. Donald F. Parker
Graduate School of Business Administr[illegible]
University of Michigan
Ann Arbor, MI 4810[illegible]

1 LT Frank C. Petho, MSC, USN (Ph.D)
Code L51
Naval Aerospace Medical Research Lab[illegible]
Pensacola, FL [illegible]

Navy

1 Roger W. Remington, Ph.D
Code L52
NAMRL
Pensacola, FL 32508

1 Dr. Bernard Rimland (03B)
Navy Personnel R&D Center
San Diego, CA 92152

1 Mr. Arnold Rubenstein
Naval Personnel Support Technology
Naval Material Command (03T244)
Room 1044, Crystal Plaza #5
2221 Jefferson Davis Highway
Arlington, VA 20360

1 Dr. Worth Scanland
Chief of Naval Education and Training
Code N-5
NAS, Pensacola, FL 32508

1 Dr. Sam Schiflett, SY 721
Systems Engineering Test Directorate
U.S. Naval Air Test Center
Patuxent River, MD 20670

1 Dr. Robert G. Smith
Office of Chief of Naval Operations
OP-987H
Washington, DC 20350

1 Dr. Alfred F. Smode
Training Analysis & Evaluation Group (TAEG)
Dept. of the Navy
Orlando, FL 32813

1 W. Gary Thomson
Naval Ocean Systems Center
Code 7132
San Diego, CA 92152

1 Dr. Ronald Weitzman
Code 54 WZ
Department of Administrative Sciences
U. S. Naval Postgraduate School
Monterey, CA 93940

Navy

1 Dr. Robert Wisher
Code 309
Navy Personnel R&D Center
San Diego, CA 92152

1 DR. MARTIN F. WISKOFF
NAVY PERSONNEL R& D CENTER
SAN DIEGO, CA 92152

1 Mr John H. Wolfe
Code P310
U. S. Navy Personnel Research and Development Center
San Diego, CA 92152

Army

1 Technical Director
U. S. Army Research Institute for the
Behavioral and Social Sciences
5001 Eisenhower Avenue
Alexandria, VA 22333

1 HQ USAREUE & 7th Army
ODCSOPS
USAAREUE Director of GED
APO New York 09403

1 DR. RALPH DUSEK
U.S. ARMY RESEARCH INSTITUTE
5001 EISENHOWER AVENUE
ALEXANDRIA, VA 22333

1 Dr. Michael Kaplan
U.S. ARMY RESEARCH INSTITUTE
5001 EISENHOWER AVENUE
ALEXANDRIA, VA 22333

1 Dr. Milton S. Katz
Training Technical Area
U.S. Army Research Institute
5001 Eisenhower Avenue
Alexandria, VA 22333

1 Dr. Harold F. O'Neil, Jr.
Attn: PERI-OK
Army Research Institute
5001 Eisenhower Avenue
Alexandria, VA 22333

1 Dr. Robert Sasmor
U. S. Army Research Institute for the
Behavioral and Social Sciences
5001 Eisenhower Avenue
Alexandria, VA 22333

1 Dr. Frederick Steinheiser
U. S. Army Research Institute
5001 Eisenhower Avenue
Alexandria, VA 22333

1 Dr. Joseph Ward
U.S. Army Research Institute
5001 Eisenhower Avenue
Alexandria, VA 22333

Air Force

1 Air University Library
AUL/LSE 76/443
Maxwell AFB, AL 36112

1 Dr. Earl A. Alluisi
HQ, AFHRL (AFSC)
Brooks AFB, TX 78235

1 Dr. Genevieve Haddad
Program Manager
Life Sciences Directorate
AFOSR
Bolling AFB, DC 20332

1 Dr. Ronald G. Hughes
AFHRL/OTR
Williams AFB, AZ 85224

1 Dr. Ross L. Morgan (AFHRL/LR)
Wright -Patterson AFB
Ohio 45433

1 Dr. Malcolm Ree
AFHRL/MP
Brooks AFB, TX 78235

1 Dr. Marty Rockway
Technical Director
AFHRL(OT)
Williams AFB, AZ 58224

2 3700 TCHTW/TTGH Stop 32
Sheppard AFB, TX 76311

1 Jack A. Thorp, Maj., USAF
Life Sciences Directorate
AFOSR
Bolling AFB, DC 20332

Marines

1 H. William Greenup
Education Advisor (E031)
Education Center, MCDEC
Quantico, VA 22134

1 Headquarters, U. S. Marine Corps
Code MPI-20
Washington, DC 20380

1 Special Assistant for Marine
Corps Matters
Code 100M
Office of Naval Research
800 N. Quincy St.
Arlington, VA 22217

1 DR. A.L. SLAFKOSKY
SCIENTIFIC ADVISOR (CODE RD-1)
HQ, U.S. MARINE CORPS
WASHINGTON, DC 20380

CoastGuard

1 Chief, Psychological Research Branch
U. S. Coast Guard (G-P-1/2/TP42)
Washington, DC 20593

1 Mr. Thomas A. Warm
U. S. Coast Guard Institute
P. O. Substation 18
Oklahoma City, OK 73169

Other DoD

12 Defense Technical Information Center
Cameron Station, Bldg 5
Alexandria, VA 22314
Attn: TC

1 Dr. Dexter Fletcher
ADVANCED RESEARCH PROJECTS AGENCY
1400 WILSON BLVD.
ARLINGTON, VA 22209

1 Military Assistant for Training and
Personnel Technology
Office of the Under Secretary of Defense
for Research & Engineering
Room 3D129, The Pentagon
Washington, DC 20301

Civil Govt

1 Dr. Susan Chipman
Learning and Development
National Institute of Education
1200 19th Street NW
Washington, DC 20208

1 Dr. Joseph I. Lipson
SEDR W-638
National Science Foundation
Washington, DC 20550

1 William J. McLaurin
Rm. 301, Internal Revenue Service
2221 Jefferson Davis Highway
Arlington, VA 22202

1 Dr. Andrew R. Molnar
Science Education Dev.
and Research
National Science Foundation
Washington, DC 20550

1 Personnel R&D Center
Office of Personnel Managment
1900 E Street NW
Washington, DC 20415

1 Dr. H. Wallace Sinaiko
Program Director
Manpower Research and Advisory Services
Smithsonian Institution
801 North Pitt Street
Alexandria, VA [illegible]

1 Dr. Frank Withrow
U. S. Office of Education
400 Maryland Ave. SW
Washington, DC [illegible]

1 Dr. Joseph L. Young, Director
Memory & Cognitive Processes
National Science Foundation
Washington, DC [illegible]

Non Govt

1 Dr. John R. Anderson
Department of Psychology
Carnegie Mellon University
Pittsburgh, PA 15213

1 Anderson, Thomas H., Ph.D.
Center for the Study of Reading
174 Children's Research Center
51 Gerty Drive
Champiagn, IL 61820

1 Dr. John Annett
Department of Psychology
University of Warwick
Coventry CV4 7AL
ENGLAND

1 DR. MICHAEL ATWOOD
SCIENCE APPLICATIONS INSTITUTE
40 DENVER TECH. CENTER WEST
7935 E. PRENTICE AVENUE
ENGLEWOOD, CO 80110

1 1 psychological research unit
Dept. of Defense (Army Office)
Campbell Park Offices
Canberra ACT 2600, Australia

1 Dr. Alan Baddeley
Medical Research Council
Applied Psychology Unit
15 Chaucer Road
Cambridge CB2 2EF
ENGLAND

1 Dr. Patricia Baggett
Department of Psychology
University of Denver
University Park
Denver, CO 80208

1 Mr Avron Barr
Department of Computer Science
Stanford University
Stanford, CA 94305

Non Govt

1 Dr. Jackson Beatty
Department of Psychology
University of California
Los Angeles, CA 90024

1 Dr. Isaac Bejar
Educational Testing Service
Princeton, NJ 08450

1 Dr. Nicholas A. Bond
Dept. of Psychology
Sacramento State College
600 Jay Street
Sacramento, CA 95819

1 Dr. Lyle Bourne
Department of Psychology
University of Colorado
Boulder, CO 80309

1 Dr. Robert Brennan
American College Testing Programs
P. O. Box 168
Iowa City, IA 52240

1 Dr. John S. Brown
XEROX Palo Alto Research Center
3333 Coyote Road
Palo Alto, CA 94304

1 Dr. Bruce Buchanan
Department of Computer Science
Stanford University
Stanford, CA 94305

1 DR. C. VICTOR BUNDERSON
WICAT INC.
UNIVERSITY PLAZA, SUITE 10
1160 SO. STATE ST.
OREM, UT 84057

1 Dr. Pat Carpenter
Department of Psychology
Carnegie-Mellon University
Pittsburgh, PA 15213

Non Govt

1 Dr. John B. Carroll
Psychometric Lab
Univ. of No. Carolina
Davie Hall 013A
Chapel Hill, NC 27514

1 Charles Myers Library
Livingstone House
Livingstone Road
Stratford
London E15 2LJ
ENGLAND

1 Dr. William Chase
Department of Psychology
Carnegie Mellon University
Pittsburgh, PA 15213

1 Dr. Micheline Chi
Learning R & D Center
University of Pittsburgh
3939 O'Hara Street
Pittsburgh, PA 15213

1 Dr. William Clancey
Department of Computer Science
Stanford University
Stanford, CA 94305

1 Dr. Kenneth E. Clark
College of Arts & Sciences
University of Rochester
River Campus Station
Rochester, NY 14627

1 Dr. Norman Cliff
Dept. of Psychology
Univ. of So. California
University Park
Los Angeles, CA 90007

1 Dr. Allan M. Collins
Bolt Beranek & Newman, Inc.
50 Moulton Street
Cambridge, Ma 02138

Non Govt

1 Dr. Lynn A. Cooper
LRDC
University of Pittsburgh
3939 O'Hara Street
Pittsburgh, PA 15213

1 Dr. Meredith P. Crawford
American Psychological Association
1200 17th Street, N.W.
Washington, DC [illegible]

1 Dr. Kenneth B. Cross
Anacapa Sciences, Inc.
P.O. Drawer Q
Santa Barbara, CA [illegible]

1 Dr. Emmanuel [illegible]
Department of Psychology
University of Illinois
Champaign, IL 61820

1 Dr. Hubert Dreyfus
Department of Philosophy
University of California
Berkely, CA 94720

1 LCOL J. C. Eggenberger
DIRECTORATE OF PERSONNEL APPLIED RESEA[illegible]
NATIONAL DEFENCE HQ
101 COLONEL BY DRIVE
OTTAWA, CANADA K1A 0K2

1 ERIC Facility-Acquisitions
4833 Rugby Avenue
Bethesda, MD [illegible]

1 Dr. [illegible] Feigenbaum
Department of Computer Science
Stanford University
Stanford, CA [illegible]

1 [illegible] L. Ferguson
[illegible] College Testing Program
[illegible]
[illegible], IA 52240

Non Govt

1 Dr. Edwin A. Fleishman
Advanced Research Resources Organ.
Suite 900
4330 East West Highway
Washington, DC 20014

1 Dr. John R. Frederiksen
Bolt Beranek & Newman
50 Moulton Street
Cambridge, MA 02138

1 Dr. Alinda Friedman
Department of Psychology
University of Alberta
Edmonton, Alberta
CANADA T6G 2E9

1 Dr. R. Edward Geiselman
Department of Psychology
University of California
Los Angeles, CA 90024

1 DR. ROBERT GLASER
LRDC
UNIVERSITY OF PITTSBURGH
3939 O'HARA STREET
PITTSBURGH, PA 15213

1 Dr. Marvin D. Glock
217 Stone Hall
Cornell University
Ithaca, NY 14853

1 Dr. Daniel Gopher
Industrial & Management Engineering
Technion-Israel Institute of Technology
Haifa
ISRAEL

1 DR. JAMES G. GREENO
LRDC
UNIVERSITY OF PITTSBURGH
3939 O'HARA STREET
PITTSBURGH, PA 15213

1 Dr. Ron Hambleton
School of Education
University of Massachusetts
Amherst, MA 01002

Non Govt

1 Dr. Harold Hawkins
Department of Psychology
University of Oregon
Eugene OR 97403

1 Dr. Barbara Hayes-Roth
The Rand Corporation
1700 Main Street
Santa Monica, CA 90406

1 Dr. Frederick Hayes-Roth
The Rand Corporation
1700 Main Street
Santa Monica, CA 90406

1 Dr. James R. Hoffman
Department of Psychology
University of Delaware
Newark, DE 19711

1 Glenda Greenwald, Ed.
"Human Intelligence Newsletter"
P. O. Box 1163
Birmingham, MI 48012

1 Dr. Lloyd Humphreys
Department of Psychology
University of Illinois
Champaign, IL 61820

1 Library
HumRRO/Western Division
27857 Berwick Drive
Carmel, CA 93921

1 Dr. Earl Hunt
Dept. of Psychology
University of Washington
Seattle, WA 98105

1 Dr. Steven W. Keele
Dept. of Psychology
University of Oregon
Eugene, OR 97403

1 Dr. Walter Kintsch
Department of Psychology
University of Colorado
Boulder, CO 80302

Non Govt

1 Dr. David Kieras
Department of Psychology
University of Arizona
Tuscon, AZ 85721

1 Dr. Stephen Kosslyn
Harvard University
Department of Psychology
33 Kirkland Street
Cambridge, MA 02138

1 Mr. Marlin Kroger
1117 Via Goleta
Palos Verdes Estates, CA 90274

1 Dr. Jill Larkin
Department of Psychology
Carnegie Mellon University
Pittsburgh, PA 15213

1 Dr. Alan Lesgold
Learning R&D Center
University of Pittsburgh
Pittsburgh, PA 15260

1 Dr. Charles Lewis
Faculteit Sociale Wetenschappen
Rijksuniversiteit Groningen
Oude Boteringestraat
Groningen
NETHERLANDS

1 Dr. James Lumsden
Department of Psychology
University of Western Australia
Nedlands W.A. 6009
AUSTRALIA

1 Dr. Mark Miller
Computer Science Laboratory
Texas Instruments, Inc.
Mail Station 371, P.O. Box 225936
Dallas, TX 75265

1 Dr. Allen Munro
Behavioral Technology Laboratories
1845 Elena Ave., Fourth Floor
Redondo Beach, CA 90277

Non Govt

1 Dr. Donald A Norman
Dept. of Psychology C-009
Univ. of California, San Diego
La Jolla, CA 92093

1 Dr. Melvin R. Novick
356 Lindquist Center for Measurment
University of Iowa
Iowa City, IA 52242

1 Dr. Jesse Orlansky
Institute for Defense Analyses
400 Army Navy Drive
Arlington, VA 22202

1 Dr. Seymour A. Papert
Massachusetts Institute of Technology
Artificial Intelligence Lab
545 Technology Square
Cambridge, MA 02139

1 Dr. James A. Paulson
Portland State University
P.O. Box 751
Portland, OR 97207

1 MR. LUIGI PETRULLO
2431 N. EDGEWOOD STREET
ARLINGTON, VA 22207

1 Dr. Martha Polson
Department of Psychology
University of Colorado
Boulder, CO 80302

1 DR. PETER POLSON
DEPT. OF PSYCHOLOGY
UNIVERSITY OF COLORADO
BOULDER, CO 80309

1 Dr. Steven E. Poltrock
Department of Psychology
University of Denver
Denver, CO 80208

1 DR. DIANE M. RAMSEY-KLEE
R-K RESEARCH & SYSTEM DESIGN
3947 RIDGEMONT DRIVE
MALIBU, CA 90265

Non Govt

1 MINRAT M. L. RAUCH
P II 4
BUNDESMINISTERIUM DER VERTEIDIGUNG
POSTFACH 1328
D-53 BONN 1, GERMANY

1 Dr. Mark D. Reckase
Educational Psychology Dept.
University of Missouri-Columbia
4 Hill Hall
Columbia, MO 65211

1 Dr. Fred Reif
SESAME
c/o Physics Department
University of California
Berkely, CA 94720

1 Dr. Andrew M. Rose
American Institutes for Research
1055 Thomas Jefferson St. NW
Washington, DC 20007

1 Dr. Ernst Z. Rothkopf
Bell Laboratories
600 Mountain Avenue
Murray Hill, NJ 07974

1 PROF. FUMIKO SAMEJIMA
DEPT. OF PSYCHOLOGY
UNIVERSITY OF TENNESSEE
KNOXVILLE, TN 37916

1 Dr. Irwin Sarason
Department of Psychology
University of Washington
Seattle, WA 98195

1 DR. WALTER SCHNEIDER
DEPT. OF PSYCHOLOGY
UNIVERSITY OF ILLINOIS
CHAMPAIGN, IL 61820

1 Dr. Alan Schoenfeld
Department of Mathematics
Hamilton College
Clinton, NY 13323

Non Govt

1 Committee on Cognitive Research
% Dr. Lonnie R. Sherrod
Social Science Research Council
605 Third Avenue
New York, NY 10016

1 Robert S. Siegler
Associate Professor
Carnegie-Mellon University
Department of Psychology
Schenley Park
Pittsburgh, PA 15213

1 Dr. Robert Smith
Department of Computer Science
Rutgers University
New Brunswick, NJ 08903

1 Dr. Richard Snow
School of Education
Stanford University
Stanford, CA 94305

1 DR. ALBERT STEVENS
BOLT BERANEK & NEWMAN, INC.
50 MOULTON STREET
CAMBRIDGE, MA 02138

1 Dr. Thomas G. Sticht
Director, Basic Skills Division
HUMRRO
300 N. Washington Street
Alexandria,VA 22314

1 Dr. David Stone
ED 236
SUNY, Albany
Albany, NY 12222

1 DR. PATRICK SUPPES
INSTITUTE FOR MATHEMATICAL STUDIES IN
THE SOCIAL SCIENCES
STANFORD UNIVERSITY
STANFORD, CA 94305

Non Govt

1 Dr. Hariharan Swaminathan
Laboratory of Psychometric and
Evaluation Research
School of Education
University of Massachusetts
Amherst, MA 01003

1 Dr. Kikumi Tatsuoka
Computer Based Education Research
Laboratory
252 Engineering Research Laboratory
University of Illinois
Urbana, IL 61801

1 Dr. David Thissen
Department of Psychology
University of Kansas
Lawrence, KS 66044

1 Dr. John Thomas
IBM Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598

1 DR. PERRY THORNDYKE
THE RAND CORPORATION
1700 MAIN STREET
SANTA MONICA, CA 90406

1 Dr. Douglas Towne
Univ. of So. California
Behavioral Technology Labs
1845 S. Elena Ave.
Redondo Beach, CA 90277

1 Dr. J. Uhlaner
Perceptronics, Inc.
6271 Variel Avenue
Woodland Hills, CA 91364

1 Dr. Benton J. Underwood
Dept. of Psychology
Northwestern University
Evanston, IL 60201

1 Dr. William R. Uttal
University of Michigan
Institute for Social Research
Ann Arbor, MI 48106

Non Govt

1 Dr. Howard Wainer
Bureau of Social SCience Research
1990 M Street, N. W.
Washington, DC 20036

1 Dr. Phyllis Weaver
Graduate School of Education
Harvard University
200 Larsen Hall, Appian Way
Cambridge, MA 02138

1 Dr. David J. Weiss
N660 Elliott Hall
University of Minnesota
75 E. River Road
Minneapolis, MN 55455

1 Dr. Keith T. Wescourt
Information Sciences Dept.
The Rand Corporation
1700 Main St.
Santa Monica, CA 90406

1 DR. SUSAN E. WHITELY
PSYCHOLOGY DEPARTMENT
UNIVERSITY OF KANSAS
LAWRENCE, KANSAS 66044

1 Dr. Christopher Wickens
Department of Psychology
University of Illinois
Champaign, IL 61820

1 Dr. J. Arthur Woodward
Department of Psychology
University of California
Los Angeles, CA 90024